October 12, 1951.

Dr. Aaron Novick,
1125 E. 61 Street,
Chicago 37, Ill.

Dear AAron:

Just yesterday, I received a letter from Klieneberger-Nobel with the regrettable information that the conference she had been invited to in N.Y. had been called off. Therefore, she will not be able to come to this country next month as we had all hoped. Will you tell Szilard about this right away— he apparently submitted a request to Rockefeller to underwrite her visiting the Midwest. Curiously, K-N herself speculated about applying for a Rockefeller travelling grant perhaps next year.

Esther and I are awfully sorry we couldn't connect properly for a more than transient visit with you (in either direction) this last summer. We thought of driving down next weekend, but Roger Stanier write that he might pay us a visit about that time, and the following Saturday there's a local SAB meeting. But there are still a few weeks before the snow flies. Of course, we'd like it at least as well, or better, if you could tear away yourselves.

We're having a very confusing time of it trying to confirm some of K-N's work on L-forms in phage-treated B. Our T3 stocks all are behaving differently. It would help if we could have a T2 tester : i.e., a T2-resistant, sensitive to other phages, or at least to T3, for some of our peculiarities would be explained if T2 had gotten into some of the stocks. Do you have such a beast?

Not much new otherwise. I'm still screening for more cross-fertile strains (and have about 30 now), and Skaar has started serology on them. Norton is trying to "cross" S. typhi x typhimurium, with apparent success. It may be a fluke, but it rather looks as if S^r is a two step mutation: 1) from stable S^s to mutable S^s (change at a modifier), and then 2) S^s to S^r. This would explain the unusually low mutation rate. Esther and I have a little evidence that lysogenicity does not come about in a single simple step of infection (the latter also disagreeing violently with the crossing data). But mostly we're putting away our culture collection in dry tubes (after Hershey). I've had very encouraging results drying cultures direc' on silica gel, and sealing off the tubes in air, but don't dare risk th' culture collection until this technique has been tried for a year or t' Would you be interested in trying it as a trial method? It is defini the most convenient method for short term preservation, e.g., in shi cultures.

Sincerely,